

EXPERIMENTS
UPON
MAGNESIA ALBA, QUICK-LIME,
AND OTHER
ALCALINE SUBSTANCES;

By **JOSEPH BLACK, M. D.**
Professor of CHYMISTRY in the University of Edinburgh.

To which is annexed,
An ESSAY on the COLD produced by
EVAPORATING FLUIDS,

AND
Of some other means of producing COLD;

By **WILLIAM CULLEN, M. D.**
Professor of MEDICINE in the University of Edinburgh.

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It was originally obtained from a liquor called the *Mother of nitre*, which is produced in the following manner :

SALT-PETRE is separated from the brine which first affords it, or from the water with which it is washed out of nitrous earths, by the process commonly used in crystallizing salts. In this process, the brine is gradually diminished, and at length reduced to a small quantity of an unctuous bitter saline liquor, affording no more salt-petre by evaporation, but, if urged with a brisk fire, drying up into a confused mass, which attracts water strongly, and becomes fluid again when exposed to the open air.

To this liquor the workmen have given the name of the *Mother of nitre* ;
and

and Hoffman, finding it composed of the *magnesia* united to an acid, obtained a separation of these, either by exposing the compound to a strong fire, in which the acid was dissipated, and the *magnesia* remained behind, or by the addition of an alkali, which attracted the acid to itself: and this last method he recommends as the best. He likewise makes an inquiry into the nature and virtues of the powder thus prepared; and observes, that it is an absorbent earth, which joins readily with all acids, and must necessarily destroy any acidity it meets in the stomach; but that its purgative power is uncertain, for sometimes it has not the least effect of that kind. As it is a mere insipid earth, he rationally concludes it to be a purgative only when

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converted into a sort of neutral salt by an acid in the stomach, and that its effect is therefore proportional to the quantity of this acid.

ALTHOUGH *magnesia* appears from this history of it, to be a very innocent medicine ; yet, having observed that some hypochondriacs, who used it frequently, were subject to flatulencies and spasms, he seems to have suspected it of some noxious quality. The circumstances however which gave rise to his suspicion, may very possibly have proceeded from the imprudence of his patients ; who, trusting too much to *magnesia* (which is properly a palliative in that disease) and neglecting the assistance of other remedies, allowed their disorder to encrease upon them.

them. It may, indeed, be alledged, that *magnesia*, as a purgative, is not the most eligible medicine for such constitutions, as they agree best with those that strengthen, stimulate, and warm; which the saline purges, commonly used, are not observed to do. But there seems at least to be no objection to its use, when children are troubled with an acid in their stomach: for, gentle purging, in this case, is very proper; and it is often more conveniently procured by means of *magnesia*, than of any other medicine, on account of its being entirely insipid.

THE above-mentioned Author, observing, some time after, that a bitter saline liquor, similar to that obtained from the brine of salt-petre, was likewise

wife produced by the evaporation of those waters which contain common salt, had the curiosity to try if this would also yield a *magnesia*. The experiment succeeded : And he thus found out another process for obtaining this powder ; and at the same time assured himself, by experiments, that the product from both was exactly the same *.

My curiosity led me, some time ago, to inquire more particularly into the nature of *magnesia*, and especially to compare its properties with those of the other absorbent earths, of which there plainly appeared to me to be very different kinds, although commonly confounded together under one

one name. I was indeed led to this examination of the absorbent earths, partly by the hope of discovering a new sort of lime and lime-water, which might possibly be a more powerful solvent of the stone, than that commonly used; but was disappointed in my expectations.

I have had no opportunity of seeing Hoffman's first *magnesia*, or the liquor from which it is prepared; and have therefore been obliged to make my experiments upon the second.

IN order to prepare it, I at first employed the bitter saline liquor called *bittern*, which remains in the pans after the evaporation of sea-water. But as that liquor is not always easily pro-
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cured,

cured, I afterwards made use of a salt called *Epsom salt*, which is separated from the bittern by crystallization, and is evidently composed of *magnesia* and the vitriolic acid.

THERE is likewise a spurious kind of Glauber salt, which yields plenty of *magnesia*, and seems to be no other than Epsom salt, of sea-water reduced to crystals of a larger size. And common salt also affords a small quantity of this powder; because, being separated from the bittern by one hasty crystallization only, it necessarily contains a portion of that liquor.

THOSE who would prepare a *magnesia* from Epsom salt, may use the following process :

DISSOLVE

Dissolve equal quantities of Epſom ſalt, and of pearl aſhes, ſeparately, in a ſufficient quantity of water; purify each ſolution from its dregs, and mix them accurately together by violent agitation. Then make them juſt to boil over a brisk fire.

ADD now to the mixture, three or four times its quantity of hot water; after a little agitation, allow the *magneſia* to ſettle to the bottom, and decant off as much of the water as poſſible. Pour on the ſame quantity of cold water; and, after ſetting, decant it off in the ſame manner. Repeat this waſhing with the cold water ten or twelve times, or even oftner, if the *magneſia* be required perfectly pure for chemical experiments.

WHEN it is sufficiently washed, the water may be strained and squeezed from it in a linen cloth ; for very little of the *magnesia* passes through.

THE alkali in the mixture, uniting with the acid, separates it from the *magnesia* ; which, not being of itself soluble in water, must consequently appear immediately under a solid form. But the powder which thus appears, is not entirely *magnesia* ; part of it is the neutral salt, formed from the union of the acid and alkali. This neutral salt is found, upon examination, to agree in all respects with vitriolated tartar, and requires a large quantity of hot water to dissolve it. As much of it is therefore dissolved as the water can take up ; the rest is dispersed through

through the mixture, in the form of a powder. Hence the necessity of washing the *magnesia* with so much trouble ; for the first effusion of hot water is intended to dissolve the whole of the salt, and the subsequent additions of cold water to wash away this solution.

THE caution given, of boiling the mixture, is not unnecessary : if it be neglected, the whole of the *magnesia* is not accurately separated at once ; and, by allowing it to rest for some time, that powder concretes into minute grains, which, when viewed with the microscope, appear to be assemblages of needles diverging from a point. This happens more especially when the solution of the Epsom salt, and of the

the alkali, are diluted with too much water before they are mixed together. Thus, if a dram of Epsom salt, and of salt of tartar, be dissolved each in four ounces of water, and be mixed, and then allowed to rest three or four days, the whole of the *magnesia* will be formed into these grains. Or, if we filtrate the mixture soon after it is made, and heat the clear liquor which passes through, it will become turbid, and deposit a *magnesia*.

I had the curiosity to satisfy myself of the purgative power of *magnesia*, and of Hoffman's opinion concerning it, by the following easy experiment. I made a neutral salt of *magnesia*, and distilled vinegar; chusing this acid, as being, like that in weak stomachs, the product of fermentation. Six drams of this I dissolved

dissolved in water, and gave to a middle-aged man, desiring him to take it by degrees. After having taken about a third, he desisted, and purged four times in an easy and gentle manner. A woman of a strong constitution got the remainder as a brisk purgative, and it operated ten times without causing any uneasiness. The taste of this salt is not disagreeable, and it appears to be rather of the cooling, than of the acrid kind.

HAVING thus given a short sketch of the history and medical virtues of *magnesia*, I now proceed to an account of its chemical properties. By my first experiments, I intended to learn what sort of neutral salts might be obtained, by joining it to each of the vulgar acids; and the result was as follows.

MAGNESIA

MAGNESIA is quickly dissolved with violent effervescence, or explosion of air, by the acids of vitriol, nitre, and of common salt, and by distilled vinegar; the neutral saline liquors thence produced having each their peculiar properties.

THAT which is made with the vitriolic acid, may be condensed into crystals similar in all respects to Epsom salt.

THAT which is made with the nitrous, is of a yellow colour, and yields saline crystals, which retain their form in a very dry air, but melt in a moist one.

THAT which is produced by means of spirit of salt, yields no crystals; and, if

if evaporated to dryness, soon melts again when exposed to the air.

THAT which is obtained from the union of distilled vinegar with *magnesia*, affords no crystals by evaporation, but is condensed into a saline mass, which, while warm, is extremely tough and viscid, very much resembling a strong glue both in colour and consistence, and becomes brittle when cold.

By these experiments, *magnesia* appears to be a substance very different from those of the calcareous class; under which I would be understood to comprehend all those that are converted into a perfect quick-lime in a strong fire, such as lime-stone, marble, chalk, those spars and marles which

C effervesce

effervesce with aqua-fortis, all animal shells, and the bodies called *lithophyta*. All of these, by being joined with acids, yield a set of compounds which are very different from those we have just now described. Thus, if a small quantity of any calcareous matter be reduced to a fine powder, and thrown into spirit of vitriol, it is attacked by this acid with a brisk effervescence; but little or no dissolution ensues. It absorbs the acid, and remains united with it, in the form of a white powder, at the bottom of the vessel, while the liquor has hardly any taste, and shews only a very light cloud upon the addition of an alkali*.

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* Mr Margraaf has lately demonstrated, by a set of curious and accurate experiments, that
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THE same white powder is also formed when spirit of vitriol is added to a calcareous earth dissolved in any other acid; the vitriolic expelling the other acid, and joining itself to the earth by a stronger attraction; and, upon this account, the *magnesia* of seawater seems to be different from either of those described by *Hoffman*. He says expressly, that the solutions of each of his powders, or, what is equivalent, that the liquors from which they are obtained, formed a coagulum, and deposited a white powder,

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when

this powder is of the nature, and possesses the properties, of the gypseous or selenitic substances.—That such substances can be resolved into vitriolic acid and calcareous earth, and can be again composed by joining these two ingredients together. *Mem. de l'Acad. de Berlin*, ann. 1750, p. 144.

when he added the vitriolic acid*; which experiment I have often tried with the marine bittern, but without success. The coagulum thus formed in the mother of nitre, may be owing to a quantity of quick-lime contained in it; for quick-lime is used in extracting the salt-petre from its matrix. —But it is more difficult to account for the difference between Hoffman's bittern and ours, unless we will be satisfied to refer it to this, that he got his from the waters of salt-springs, which may possibly be different from those of the sea.

MAGNESIA is not less remarkably distinguished from the calcareous earths,
by

* Hoff. Op. T. 4. p. 480. & 500.

by joining it to the nitrous and vegetable acids, than to the vitriolic. Those earths, when combined with spirit of nitre, cannot be reduced to a crystalline form ; and, if they are dissolved in distilled vinegar, the mixture spontaneously dries up in a friable salt.

HAVING thus found *magnesia* to differ from the common alkaline earths, the object of my next inquiry was its peculiar degree of attraction for acids, or what was the place due to it in Mr Geoffroy's table of elective attractions.

THREE drams of *magnesia* in fine powder, an ounce of salt-ammoniac, and six ounces of water, were mixed together, and digested six days in a retort joined to a receiver.

DURING

DURING the whole time, the neck of the retort was pointed a little upwards, and the most watery part of the vapour, which was condensed there, fell back into its body. In the beginning of the experiment, a volatile salt was therefore collected, in a dry form, in the receiver, and afterwards dissolved into spirit.

WHEN all was cool, I found, in the retort, a saline liquor, some undissolved *magnesia*, and some salt-ammoniac crystallized. The saline liquor was separated from the other two, and then mixed with the alkaline spirit. A coagulum was immediately formed, and a *magnesia* precipitated from the mixture.

THE *magnesia* which had remained in the retort, when well washed and dried,

dried, weighed two scruples and fifteen grains.

WE learn, by the latter part of this experiment, that the attraction of the volatile alkali for acids is stronger than that of *magnesia*, since it separated this powder from the acid to which it was joined. But it also appears, that a gentle heat is capable of overcoming this superiority of attraction, and of gradually elevating the alkali, while it leaves the less volatile acid with the *magnesia*.

DISSOLVE a dram of any calcareous substance in the acid of nitre or of common salt, taking care that the solution be rendered perfectly neutral, or that no superfluous acid be added.

Mix

Mix with this solution, a dram of *magnesia* in fine powder, and digest it in the heat of boiling water about twenty-four hours ; then dilute the mixture with double its quantity of water, and filtrate. The greatest part of the earth now left in the filtre is calcareous ; and the liquor which passed through, if mixed with a dissolved alkali, yields a white powder, the largest portion of which is a true *magnesia*.

From this experiment it appears, that an acid quits a calcareous earth to join itself to *magnesia* ; but the exchange being performed slowly, some of the *magnesia* is still undissolved, and part of the calcareous earth remains yet joined to the acid.

WHEN

WHEN a small quantity of *magnesia* is thrown into a solution of the corrosive sublimate of mercury, it soon separates part of the mercury, in the form of a dark red powder, and is itself dissolved.

IMAGINING that I perceived some resemblance between the properties of *magnesia* and those of alkalis, I was led to try what change this substance would suffer from the addition of quicklime, which alters in such a peculiar manner the alkaline salts.

TWENTY-SEVEN grains of *magnesia* in fine powder were mixed with eighteen ounces of lime-water in a flask, which was corked close, and shaken frequently for four days. During this
D time,

time, I frequently dipped into it little bits of paper, which were coloured with the juice of violets ; and these became green as soon as they touched the water, until the fourth day, when their colour did not seem to be altered. The water being now poured off, was entirely insipid, and agreed in every chemical trial with pure water. The powder, after being perfectly well dried, weighed thirty-seven grains. It did not dissolve entirely in spirit of vitriol ; but, after a brisk effervescence, part of it subsided, in the same manner as the calcareous earths, when mixed with this acid.

WHEN I first tried this experiment, I was at the trouble of digesting the mixture in the heat of boiling water, and

and did not then know that it would succeed in the heat of the air. But Dr Alston, who has obliged the world with many curious and useful discoveries on the subject of quick-lime, having had occasion to repeat it, I learned from him, that heat is not necessary : And he has moreover added an useful purpose, to which this property of *magnesia* may be applied ; I mean the sweetening of water at sea, with which lime may have been mixed, to prevent its putrefaction.

THAT part of the dried powder which does not dissolve in spirit of vitriol, consists of the lime separated from the water.

QUICK-LIME itself is also rendered

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mild

mild by *magnesia*, if these two are well rubbed together, and infused with a small quantity of water.

By the following experiments, I proposed to know whether this substance could be reduced to a quick-lime.

AN ounce of *magnesia* was exposed in a crucible, for about an hour, to such a heat as is sufficient to melt copper. When taken out, it weighed three drams and one scruple, or had lost $\frac{7}{12}$ of its former weight.

I repeated, with the *magnesia* prepared in this manner, most of those experiments I had already made upon it before calcination, and the result was as follows—

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It dissolves in all the acids; and with these composes salts exactly similar to those described in the first set of experiments: But, what is particularly to be remarked, it is dissolved without any the least degree of effervescence.

It slowly precipitates the corrosive sublimate of mercury, in the form of a black powder.

It separates the volatile alkali in salt-ammoniac from the acid, when it is mixed with a warm solution of that salt. But it does not separate an acid from a calcareous earth, nor does it introduce the least change upon lime-water.

LASTLY,

LASTLY, when a dram of it is digested with an ounce of water in a bottle for some hours, it does not make any the least change in the water. The *magnesia*, when dried, is found to have gained ten grains; but it neither effervesces with acids, nor does it sensibly affect lime-water.

OBSERVING *magnesia* to lose such a remarkable proportion of its weight in the fire, my next attempts were directed to the investigation of this volatile part; and, among other experiments, the following seemed to throw some light upon it——

THREE ounces of *magnesia* were distilled in a glass retort and receiver, the fire being gradually increased until the
magnesia

magnesia was obscurely red hot. When all was cool, I found only five drams of a whitish water in the receiver, which had a faint smell of the spirit of harts-horn, gave a green colour to the juice of violets, and rendered the solutions of corrosive sublimate, and of silver, very slightly turbid. But it did not sensibly effervesce with acids.

THE *magnesia*, when taken out of the retort, weighed an ounce, three drams, and thirty grains, or had lost more than the half of its weight. It still effervesced pretty briskly with acids, though not so strongly as before this operation.

THE fire should have been raised here to the degree requisite for the perfect

perfect calcination of *magnesia*. But, even from this imperfect experiment, it is evident, that, of the volatile parts contained in that powder, a small proportion only is water ; the rest cannot, it seems, be retained in vessels, under a visible form. Chemists have often observed, in their distillations, that part of a body has vanished from their senses, notwithstanding the utmost care to retain it ; and they have always found, upon further inquiry, that subtile part to be air, which having been imprisoned in the body, under a solid form, was set free, and rendered fluid and elastic by the fire. We may therefore safely conclude, that the volatile matter lost in the calcination of *magnesia*, is mostly air ; and hence the calcined *magnesia* does not emit air, or
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make an effervescence when mixed with acids.

THE water, from its properties, seems to contain a small portion of volatile alkali, which was probably formed from the earth, air, and water, or from some of these combined together ; and perhaps also from a small quantity of inflammable matter, which adhered accidentally to the *magnesia*. Whenever chymists meet with this salt, they are inclined to ascribe its origin to some animal or putrid vegetable substance ; and this they have always done, when they obtained it from the calcareous earths, all of which afford a small quantity of it. There is, however, no doubt, that it can sometimes be produced independently

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of

of any such mixture, since many fresh vegetables, and tartar, afford a considerable quantity of it. And how can it, in the present instance, be supposed, that any animal or vegetable matter adhered to the *magnesia*, while it was dissolved by an acid, separated from this by an alkali, and washed with so much water?

Two drams of *magnesia* were calcined in a crucible, in the manner described above, and thus reduced to two scruples and twelve grains. This calcined *magnesia* was dissolved in a sufficient quantity of spirit of vitriol, and then again separated from the acid by the addition of an alkali, of which a large quantity is necessary for this purpose. The *magnesia* being very
well

well washed and dried, weighed one dram and fifty grains. It effervesced violently, or emitted a large quantity of air, when thrown into acids; formed a red powder, when mixed with a solution of sublimate; separated the calcareous earths from an acid, and sweetened lime-water: and had thus recovered all those properties which it had but just now lost by calcination. Nor had it only recovered its original properties, but acquired besides an addition of weight, nearly equal to what had been lost in the fire; and as it is found to effervesce with acids, part of the addition must certainly be air.

THIS air seems to have been furnished by the alkali, from which it was separated by the acid; for Dr Hales

has clearly proved, that alkaline salts contain a large quantity of fixed air, which they emit in great abundance when joined to a pure acid. In the present case, the alkali is really joined to an acid, but without any visible emission of air: and yet the air is not retained in it; for the neutral salt, into which it is converted, is the same in quantity, and in every other respect, as if the acid employed had not been previously saturated with *magnesia*, but offered to the alkali in its pure state, and had driven the air out of it in their conflict. It seems therefore evident, that the air was forced from the alkali by the acid, and lodged itself in the *magnesia*.

THESE considerations led me to try

a few experiments, whereby I might know what quantity of air is expelled from an alkali, or from *magnesia*, by acids.

Two drams of a pure fixed alkaline salt, and an ounce of water, were put into a Florentine flask, which, together with its contents, weighed two ounces and two drams. Some oil of vitriol diluted with water was dropt in, until the salt was exactly saturated; which it was found to be, when two drams, two scruples, and three grains of this acid had been added. The phial with its contents now weighed two ounces, four drams, and fifteen grains. One scruple, therefore, and eight grains, were lost during the ebullition; of which a trifling portion may be water,
or

or something of the same kind; the rest is air.

THE celebrated Homberg has attempted to estimate the quantity of solid salt contained in a determined portion of the several acids. He saturated equal quantities of an alkali with each of them; and, observing the weight which the alkali had gained, after being perfectly dried, took this for the quantity of solid salt contained in that share of the acid which performed the saturation. But we learn from the above experiment, that his estimate was not accurate; because the alkali loses weight, as well as gains it.

Two drams of *magnesia*, treated exactly as the alkali in the last experiment,

ment, were just dissolved by four drams, one scruple, and seven grains of the same acid liquor, and lost one scruple and sixteen grains by the ebullition.

Two drams of *magnesia* were reduced, by the action of a violent fire, to two scruples and twelve grains, with which the same process was repeated as in the two last experiments: Four drams, one scruple, and two grains of the same acid, were required to compleat the solution, and no weight was lost in the experiment.

As in the separation of the volatile from the fixed parts of bodies, by means of heat, a small quantity of the latter is generally raised with the former;

mer; so the air and water originally contained in the *magnesia*, and afterwards dissipated by the fire, seem to have carried off a small part of the fixed earth of this substance. This is probably the reason why calcined *magnesia* is saturated with a quantity of acid somewhat less than what is required to dissolve it before calcination: and the same may be assigned as one cause which hinders us from restoring the whole of its original weight by solution and precipitation.

I took care to dilute the vitriolic acid, in order to avoid the heat and ebullition which it would otherwise have excited in the water; and I chose a Florentine flask, on account of its lightness, capacity, and shape, which is peculiarly

peculiarly adapted to the experiment : for, the vapours raised by the ebullition, circulated for a short time thro' the wide cavity of the phial; but were soon collected upon its sides, like dew, and none of them seemed to reach the neck, which continued perfectly dry to the end of the experiment.

WE now perceive the reason why crude and calcined *magnesia*, which differ in many respects from one another, agree however in composing the same kind of salt, when dissolved in any particular acid; for the crude *magnesia* seems to differ from the calcined chiefly by containing a considerable quantity of air, which air is unavoidably dissipated and lost during the dissolution.

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From our experiments it seems probable, that the increase of weight, which some metals acquire, by being first dissolved in acids, and then separated from them again by alkalis, proceeds from air furnished by the alkalis : and that, in the *aurum fulminans*, which is prepared by the same means, this air adheres to the gold, in such a peculiar manner, that, in a moderate degree of heat, the whole of it recovers its elasticity in the same instant of time ; and thus, by the violent shock which it gives to the air around, produces the loud crack or fulmination of this powder. Those who will imagine the explosion of such a minute portion of fixed air, as can reside in the *aurum fulminans*, to be insufficient for the excessive loudness

ness of the noise, will consider, that it is not a large quantity of motion communicated to the air, but rather a smart stroke, which produces sound; and that the explosion of but a few particles of fixed air, may be capable of causing a loud noise, provided they all recover their spring suddenly, and in the same instant.

THE above experiments lead us also to conclude, that volatile alkalis, and the common absorbent earths, which lose their air by being joined to acids, but shew evident signs of their having recovered it when separated from them by alkalis, received it from these alkalis which lost it in the instant of their joining with the acid.

THE following are a few experiments upon three of the absorbent earths, made in order to compare them with one another, and with *magnesia*.

SUSPECTING that *magnesia* might possibly be no other than a common calcareous earth, which had changed its nature by having been previously combined with an acid, I saturated a small quantity of chalk with the muriatic acid, separated the acid from it again by means of a fixed alkali, and carefully washed away the whole of the salt.

THE chalk, when dried, was not found to have suffered any alteration; for it effervesced with the vitriolic acid, but did not dissolve in it; and, when exposed

exposed to a violent fire, was converted into a quick-lime, in all respects similar to that obtained from common chalk.

IN another experiment of the same kind, I used the vitriolic acid with the same event.

ANY calcareous matter reduced to a fine powder, and thrown into a warm solution of allum, immediately raises a brisk effervescence. But the powder is not dissolved; it is rather increased in bulk: and if the addition be repeated until it is no longer accompanied with effervescence, the liquor loses all taste of the allum, and yields only a very light cloud upon the admixture of an alkali.

FROM

FROM this experiment we learn, that acids attract the calcareous earths more strongly than they do the earth of allum ; and as the acid in this salt is exactly the same with the vitriolic, it composes with the calcareous earth a neutral substance, which is very difficultly soluble in water, and therefore falls down to the bottom of the vessel, along with the earth of allum, which is deprived of its acid. The light cloud formed by the alkali, proceeds from the minute portion of the calcareous compound which saturates the water.

THE earth of animal bones, when reduced to a fine powder, and thrown into a diluted vitriolic acid, gradually absorbs the acid, in the same manner
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as the calcareous earths, but without any remarkable effervescence. When it is added to the nitrous, or to the muriatic acid, it is slowly dissolved. The compound liquor thence produced, is extremely acrid, and still changes the colour of the juice of violets to a red, even after it is fully saturated with the absorbent. Distilled vinegar has little or no effect upon this earth; for, after a long digestion, it still retains its sour taste, and gives only a light cloud upon the addition of an alkali.

By dropping a dissolved fixed alkali into a warm solution of allum, I obtained the earth of this salt; which, after being well washed and dried, was found to have the following properties:

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It is dissolved, in every acid, but very slowly, unless assisted by heat. The several solutions, when thoroughly saturated, are all astringent, with a slight degree of an acid taste; and they also agree with a solution of allum, in this, that they give a red colour to the infusion of turnsol.

NEITHER this earth, nor that of animal bones, can be converted into quick-lime by the strongest fire, nor do they suffer any change worth notice. Both of them seem to attract acids but weakly, and to alter their properties less when united to them, than the other absorbents.

P A R T II.

IN reflecting afterwards upon these experiments, an explication of the nature of lime offered itself, which seemed to account, in an easy manner, for most of the properties of that substance.

It is sufficiently clear, that the calcareous earths in their native state, and that the alkalis and *magnesia* in their ordinary condition, contain a large quantity of fixed air ; and this air certainly adheres to them with considerable force, since a strong fire is necessary to separate it from *magnesia* ; and the strongest is not sufficient to expel it entirely from fixed alkalis, or

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take away their power of effervescing with acid salts.

THESE considerations led me to conclude, that the relation between fixed air and alkaline substances, was somewhat similar to the relation between these and acids: that as the calcareous earths and alkalis attract acids strongly, and can be saturated with them, so they also attract fixed air, and are, in their ordinary state, saturated with it: and, when we mix an acid with an alkali, or with an absorbent earth, that the air is then set at liberty, and breaks out with violence; because the alkaline body attracts it more weakly than it does the acid, and because the acid and air cannot both be joined to the same body at the same time.

I ALSO imagined, that when the calcareous earths are exposed to the action of a violent fire, and are thereby converted into quick-lime, they suffer no other change in their composition, than the loss of a small quantity of water, and of their fixed air. The remarkable acrimony which we perceive in them after this process, was not supposed to proceed from any additional matter received in the fire, but seemed to be an essential property of the pure earth, depending on an attraction for those several substances which it then became capable of corroding or dissolving; which attraction had been insensible as long as the air adhered to the earth, but discovered itself upon the separation.

THIS supposition was founded upon an observation of the most frequent consequences of combining bodies in chymistry. Commonly, when we join two bodies together, their acrimony or attraction for other substances becomes immediately either less perceivable, or entirely insensible ; although it was sufficiently strong and remarkable before their union, and may be rendered evident again by disjoining them. A neutral salt, which is composed of an acid and alkali, does not possess the acrimony of either of its constituent parts. It can easily be separated from water, has little or no effect upon metals, is incapable of being joined to inflammable bodies, and of corroding and dissolving animals and vegetables ; so that the attraction both
of

of the acid and alkali for these several substances, seems to be suspended till they are again separated from one another.

CRUDE lime was therefore considered as a peculiar acrid earth, rendered mild by its union with fixed air; and quick-lime as the same earth, in which, by having separated the air, we discover that acrimony or attraction for water, for animal, vegetable, and for inflammable substances.

THAT the calcareous earths really lose a large quantity of air when they are burnt to quick-lime, seems sufficiently proved by an experiment of Mr Margraaf*, an exceedingly accurate and

* Mem. de l'Acad. de Berlin, ann. 1748, p. 57.

and judicious chymist. He subjected eight ounces of osteocolla to distillation in an earthen retort, finishing his process with the most violent fire of a reverberatory, and caught in the receiver only two drams of water, which, by its smell and properties, shewed itself to be slightly alkaline. He does not tell us the weight of the osteocolla remaining in the retort, and only says that it was converted into quick-lime; but, as no calcareous earth can be converted into quick-lime, or bear the heat which he applied, without losing above a third of its weight, we may safely conclude, that the loss in his experiment was proportional, and proceeded chiefly from the dissipation of fixed air.

ACCORDING to our theory, the relation

lation of the calcareous earth to air and water, appeared to agree with the relation of the same earth to the vitriolic and vegetable acids. As chalk, for instance, has a stronger attraction for the vitriolic, than for the vegetable acid; and is dissolved with more difficulty when combined with the first, than when joined to the second; so it also attracts air more strongly than water, and is dissolved with more difficulty when saturated with air, than when compounded with water only.

A calcareous earth, deprived of its air, or in the state of quick-lime, greedily absorbs a considerable quantity of water, becomes soluble in that fluid, and is then said to be slaked; but as soon as it meets with fixed air, it is supposed

supposed to quit the water, and join itself to the air, for which it has a superior attraction, and is therefore restored to its first state of mildness and insolubility in water.

WHEN flaked lime is mixed with water, the fixed air in the water is attracted by the lime, and saturates a small portion of it, which then becomes again incapable of dissolution; but part of the remaining flaked lime is dissolved, and composes lime-water.

If this fluid be exposed to the open air, the particles of quick-lime which are nearest the surface, gradually attract the particles of fixed air which float in the atmosphere. But, at the same time that a particle of lime is thus saturated

saturated with air, it is also restored to its native state of mildness and insolubility; and as the whole of this change must happen at the surface, the whole of the lime is successively collected there, under its original form of an insipid calcareous earth, called the cream or crusts of lime-water.

WHEN quick-lime itself is exposed to the open air, it absorbs the particles of water, and of fixed air, which come within its sphere of attraction. As it meets with the first of these in greatest plenty, the greatest part of it assumes the form of slaked lime; the rest is restored to its original state: and if it be exposed for a sufficient length of time, the whole of it is gradually saturated with air, to which the water as gradually yields its place.

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WE have already shewn, by experiment, that *magnesia alba* is a compound of a peculiar earth and fixed air. When this substance is mixed with lime-water, the lime shews a stronger attraction for fixed air, than that of the earth of *magnesia*; the air leaves this powder, to join itself to the lime. And as neither the lime when saturated with air, nor the *magnesia* when deprived of it, are soluble in water, the lime-water becomes perfectly pure and insipid, the lime which it contained being mixed with the *magnesia*. But if the *magnesia* be deprived of air, by calcination, before it is mixed with the lime-water, this fluid suffers no alteration.

If quick-lime be mixed with a dissolved

solved alkali, it likewise shews an attraction for fixed air, superior to that of the alkali. It robs this salt of its air, and thereby becomes mild itself; while the alkali is consequently rendered more corrosive, or discovers its natural degree of acrimony, or strong attraction for water, and for bodies of the inflammable, and of the animal and vegetable kind; which attraction was less perceivable, as long as it was saturated with air. And the volatile alkali, when deprived of its air, besides this attraction for various bodies, discovers likewise its natural degree of volatility, which was formerly somewhat repressed, by the air adhering to it, in the same manner as it is repressed by the addition of an acid.

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THIS account of lime and alkalis recommended itself by its simplicity, and by affording an easy solution of many *phænomena*; but appeared, upon a nearer view, to be attended with consequences that were so very new and extraordinary, as to render suspicious the principles from which they were drawn.

I resolved however to examine, in a particular manner, such of these consequences as were the most unavoidable, and found the greatest number of them might be reduced to the following propositions——

I. If we only separate a quantity of air from lime and alkalis, when we render them caustic, they will be found

found to lose part of their weight in the operation, but will saturate the same quantity of acid as before, and the saturation will be performed without effervescence.

II. If quick-lime be no other than a calcareous earth deprived of its air, and whose attraction for fixed air is stronger than that of alkalis, it follows, that, by adding to it a sufficient quantity of alkali saturated with air, the lime will recover the whole of its air, and be entirely restored to its original weight and condition : And it also follows, that the earth separated from lime-water by an alkali, is the lime which was dissolved in the water, now restored to its original mild and insoluble state.

III.

III. IF it be supposed that flaked lime does not contain any parts which are more fiery, active, or subtile than others, and by which chiefly it communicates its virtues to water, but that it is an uniform compound of lime and water ; it follows, that as part of it can be dissolved in water, the whole of it is also capable of being dissolved.

IV. If the acrimony of the caustic alkali does not depend on any part of the lime adhering to it, a caustic or soap-ley will consequently be found to contain no lime, unless the quantity of lime employed in making it were greater than what is just sufficient to extract the whole air of the alkali; for then as much of the superfluous quicklime might possibly be dissolved by the ley as would be dissolved by pure water,

ter, or the ley would contain as much lime as lime-water does.

V. WE have shewn, in the former experiments, that absorbent earths lose their air when they are joined to an acid: but recover it, if separated again from that acid, by means of an ordinary alkali; the air passing from the alkali to the earth, at the same time that the acid passes from the earth to the alkali.

IF the caustic alkali, therefore, be destitute of air, it will separate *magnesia* from an acid, under the form of a *magnesia* free of air, or which will not effervesce with acids; and the same caustic alkali will also separate a calcareous earth from acids, under the form of a calcareous earth destitute of air, but
saturated

saturated with water, or under the form of slaked lime.

THESE were all necessary conclusions from the above suppositions. Many of them appeared too improbable, to deserve any further attention: Some, however, I found, upon reflection, were already seconded by experience. Thus Hoffman has observed, that quick-lime does not effervesce with spirit of vitriol*; and it is well known, that the caustic spirit of urine, or of salt ammoniac, does not emit air, when mixed with acids. This consideration excited my curiosity, and determined me to inquire into the truth of them all by way of experiment. I therefore engaged myself in a set of trials, the history of which is here subjoined.

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* Hoff. Op. T. 4. p. 480.

Some new facts are likewise occasionally mentioned; and here it will be proper to inform the reader, that I have never mentioned any, without satisfying myself of their truth by experiment, tho' I have sometimes taken the liberty to neglect describing the experiments when they seemed sufficiently obvious.

DESIRING to know how much of an acid a calcareous earth will absorb, and what quantity of air is expelled during the dissolution, I saturated two drams of chalk with diluted spirit of salt, and used the Florentine flask, as related in a similar experiment upon *magnesia*. Seven drams and one grain of the acid finished the dissolution, and the chalk lost two scruples and eight grains of air.

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THIS experiment was necessary before the following, by which I proposed to enquire into the truth of the first proposition, so far as it relates to quick-lime.

Two drams of chalk were converted into a perfect quick-lime, and lost two scruples and twelve grains in the fire. This quick-lime was slaked, or reduced to a milky liquor, with an ounce of water, and then dissolved, in the same manner, and with the same acid, as the two drams of chalk in the preceding experiment. Six drams, two scruples, and fourteen grains of the acid, finished the saturation, without any sensible effervescence, or loss of weight.

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It therefore appears from these experiments, that no air is separated from quick-lime by an acid, and that chalk saturates nearly the same quantity of acid after it is converted into quick-lime, as before.

WITH respect to the second proposition, I tried the following experiments —

A piece of perfect quick-lime, made from two drams of chalk, and which weighed one dram and eight grains, was reduced to a very fine powder, and thrown into a filtrated mixture of an ounce of a fixed alkaline salt and two ounces of water. After a slight digestion, the powder, being well washed and dried, weighed one dram and fifty-

eight grains. It was similar in every trial to a fine powder of ordinary chalk; and was therefore saturated with air, which must have been furnished by the alkali.

A dram of pure salt of tartar was dissolved in fourteen pounds of lime-water, and the powder thereby precipitated, being carefully collected and dried, weighed one-and-fifty grains. When exposed to a violent fire, it was converted into a true quick-lime, and had every other quality of a calcareous earth.

THIS experiment was repeated with the volatile alkali, and also with the fossil or alkali of sea-salt, and exactly with the same event.

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THE third proposition had less appearance of probability than the foregoing; but, as an accurate experiment was the only test of its truth, I reduced eight grains of perfect quick-lime made of chalk, to an exceedingly subtile powder, by slaking it in two drams of distilled water boiling hot, and immediately threw the mixture into eighteen ounces of distilled water in a flask. After shaking it, a light sediment, which floated through the liquor, was allowed to subside; and this, when collected with the greatest care, and dried, weighed, as near as I could guess, one third of a grain. The water tasted strongly of the lime, had all the qualities of lime-water, and yielded twelve grains of precipitate, upon the addition of salt of tartar. In repeating this experiment,

periment, the quantity of sediment was sometimes less than the above, and sometimes amounted to half a grain. It consisted partly of an earth which effervesced violently with *aqua-fortis*, and partly of an ochry powder, which would not dissolve in that acid. The ochry powder, as it usually appears in chalk to the eye in the form of veins running through its substance, must be considered only as an accidental or foreign admixture: and with respect to the minute portion of alkaline earth, which composed the remainder of the sediment, it cannot be supposed to have been originally different from the rest, and incapable, from its nature, of being converted into quick-lime, or of being dissolved in water; it seems rather to have consisted

consisted of a small part of the chalk in its mild state, or saturated with air, which had either remained for want of a sufficient fire to drive it out entirely, or had been furnished by the distilled water,

I indeed expected to see a much larger quantity of sediment produced from the lime, on account of the air which water constantly contains; and, with a view to know whether water retains its air when fully saturated with lime, a lime-water was made as strong as possible; four ounces of which were placed under the receiver of an air-pump, together with four ounces of common water in a phial of the same size; and, upon exhausting the receiver, without heating the
phials,

phials, the air arose from each, in nearly the same quantity: From whence it is evident, that the air which quick-lime attracts, is of a different kind from that which is mixed with water. And that it is also different from common elastic air, is sufficiently proved by daily experience; for lime-water, which soon attracts air, and forms a crust when exposed in open and shallow vessels, may be preserved, for any time, in bottles which are but slightly corked, or closed in such a manner as would allow free access to elastic air, were a vacuum formed in the bottle. Quicklime, therefore, does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, which is dispersed through

through the atmosphere, either in the shape of an exceedingly subtle powder, or more probably in that of an elastic fluid. To this I have given the name of *fixed air*, and perhaps very improperly ; but I thought it better to use a word already familiar in philosophy, than to invent a new name, before we be more fully acquainted with the nature and properties of this substance, which will probably be the subject of my further inquiry.

It is, perhaps, needless to mention here, that the calcareous substances used in making the above experiments should be of the purest kind, and burnt with the utmost violence of heat, if we would be sure of converting them into perfect quick-lime. I therefore made

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use of chalk, burnt in a small covered crucible, with the fiercest fire of a blacksmith's forge, for half an hour, and found it necessary to employ, for this purpose, a crucible of the Austrian kind, which resemble black lead; for, if any calcareous substance be heated to such a degree in an ordinary or Hessian crucible, the whole of it is melted down, together with part of the vessel, into glass.

I NOW prepared to inquire into the properties of the caustic alkali; in order to which, I made a caustic or soap-ley in the following manner—

TWENTY-SIX ounces of very strong quick-lime, made of chalk, were slaked or reduced to a sort of fluid paste, with

with eleven pounds of boiling water, and then mixed in a glass vessel with eighteen ounces of a pure fixed alkaline salt, which had been first dissolved in two pounds and a half of water. This mixture was shaken frequently for two hours, when the action of the lime upon the alkali was supposed to be over, and nothing remained but to separate them again from one another. I therefore added 12 pounds of water, stirred up the lime, and, after allowing it to settle again, poured off as much of the clear ley as possible.

THE lime and alkali were mixed together, under the form of a very thick milky liquor, or fluid paste; because they are thus kept in perpetual contact and equal mixture until they have

acted sufficiently upon one another :
 whereas, in the common way of using
 a larger quantity of water, the lime lies
 for the most part at bottom, and, tho'
 stirred up ever so often, cannot exert
 its influence so fully upon the alkali,
 which is uniformly diffused thro' eve-
 ry part of the liquor.

THE above ley was found upon trial
 to be saturated by acids, without the
 least effervescence or diminution of
 weight.

It was now proper to examine whe-
 ther the alkali suffered any loss in be-
 coming caustic, which I proposed to at-
 tempt by ascertaining the strength of
 the ley, or the quantity of salt which a
 given portion of it contained ; from
 which,

which, by computation, some imperfect knowledge might be obtained of the quantity of caustic produced from the eighteen ounces of mild salt.

I therefore evaporated some of my ley; but soon perceived that no certain judgment could be formed of its strength in this way; because it always absorbed a considerable quantity of air during the evaporation, and the dried salt made a pretty brisk effervescence with acids, so that the ley appeared stronger than it really was; and yet, upon proceeding in the estimate from this rude and unfair trial, it appeared that the salt had lost above a sixth in becoming caustic, and the quantity of acid saturated by two drams of it, was to the quantity of acid saturated by
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two drams of salt of tartar, nearly as fix to five.

THESE experiments are therefore agreeable to that part of the second proposition which relates to the caustic alkali.

UPON farther examining what changes the alkali had undergone, I found that the ley gave only an exceeding faint milky hue to lime-water; because the caustic alkali wants that air by which salt of tartar precipitates the lime. When a few ounces of it were exposed in an open shallow vessel for four-and-twenty hours, it imbibed a small quantity of air, and made a slight effervescence with acids. After a fortnight's exposure in the same manner,
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it became entirely mild, effervesced as violently with acids, and had the same effect upon lime-water, as a solution of an ordinary alkali. It likewise agrees with lime-water, in this respect, that it may be kept in close vessels, or even in bottles which are but slightly covered, for a considerable time, without absorbing a sensible quantity of air.

IN order to know how much lime it contained, I evaporated ten ounces in a small silver dish over a lamp, and melted the salt, after having dissipated the water *.

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* This evaporation was performed in a silver dish, on account of the acrimony of the salt; which is so very great, that, having once evaporated a part of the same ley in a bowl

THE caustic thus produced, was dissolved again in a small quantity of water, and deposited a trifling portion of sediment, which I imagined at first to be lime; but, finding that it could easily be dissolved in a little more water, concluded it to be a vitriolated tartar, which always accompanies the fixed alkali of vegetables.

I then saturated the solution of the caustic salt with spirit of vitriol, expecting thus to detect the lime; because that acid precipitates a calcareous earth from its ordinary solutions. During the saturation, a large quantity

bowl of English earthen or stone-ware, and melted the caustic with a gentle heat, it corroded and dissolved a part of the bowl, and left the inside of it pitted with small holes.

tity of white powder was formed ; but this likeways turned out to be a vi-
triated tartar, which had appeared in
the form of a powder, because there
was not enough of water in the mix-
ture to dissolve it.

LASTLY, I exposed a few ounces of
the ley in an open shallow vessel, so
long that the alkali lost the whole of
its causticity, and seemed entirely re-
stored to the state of an ordinary fix-
ed alkali ; but it did not however de-
posite a single atom of lime. And,
to assure myself that my caustic-ley
was not of a singular kind, I repeated
the same experiments with an ordinary
soap-ley, and with one made by mix-
ing one part of a pure fixed alkaline
salt with three parts of common stone-
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lime fresh flaked and sifted; nor could I discover any lime in either. The first of these contained a small quantity of brimstone, and was far from being perfectly caustic, for it made a pretty brisk effervescence with acids; but the last was so entirely deprived of its air, that it did not diminish in the least the transparency of lime-water.

THESE experiments seem therefore to support the fourth proposition, and to shew that the caustic alkali does not contain any lime.

As it seems probable, from the quickness and ease wherewith the alkali was rendered caustic, that more lime had been employed than what was just sufficient to extract the whole of its air,

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we are surpris'd to find, that little or none of the superfluous quick-lime was dissolved by the water. But this *phænomenon* will become less surpris'ing, by comparing it with some similar instances in chemistry. Water may be made to deposite a salt, by the admixture of a substance which it attracts more strongly than it does that salt; such as spirit of wine. And quick-lime itself may be separated from water, upon the same principle: for, if that spirit is added to an equal quantity of lime-water, the mixture becomes turbid, and deposite a sediment, which, when separated and dissolved again in distilled water, composes lime-water. We may therefore refer the above *phænomenon* with respect to the ley, to the same cause with these; and say, that the water did not

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dissolve the lime, because it already contained a caustic alkali, for which it has a superior attraction.

I also rendered the volatile alkali caustic, in order to examine what change it suffered in the operation; and obtained an exceedingly volatile and acrid spirit, which neither effervesced with acids, nor altered in the least the transparency of lime-water; and, although very strong, was lighter than water, and floated upon it like spirit of wine.

I next enquired into the truth of the fifth proposition, in the following manner——

Two drams of Epsom salt were dissolved in a small quantity of water,
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and thrown into two ounces of the caustic ley: the mixture instantly became thick, like a decoction of starch or barley, by the *magnesia*, which was precipitated. I then added spirit of vitriol by degrees, until the mixture became perfectly clear, or the whole of the *magnesia* was again dissolved; which happened without any effervescence or emission of air.

HALF an ounce of chalk was dissolved in spirit of salt, the quantity of which was so adjusted, that the mixture was not acid in the least degree; and the solution was thrown into twelve ounces of the caustic ley; which quantity I found, by experiment, to be sufficient for precipitating almost the whole of the chalk. I now filtrated
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this turbid liquor, and laid the powder remaining in the paper upon a chalk stone, in order to draw as much of the water from it as possible, and thereby reduce it to the form of a more dense and heavy powder, that it might subside the more perfectly in the following part of the experiment. I then mixed it with about twenty ounces of pure water in a flask, and, after allowing the powder to subside, poured off the water, which had all the qualities of lime-water. And I successively converted eight waters more into lime-water, seven of these in the same quantity, and with the same management, as the first. The eighth was likewise in the same quantity; but I allowed it to remain with the chalk, and shook it frequently for two days. This, after being filtrated,

ed, formed a cream or crust upon its surface when exposed to the air; changed the colour of the juice of violets into green; separated an orange-coloured powder from a solution of corrosive sublimate; became turbid upon the addition of an alkali; was entirely sweetened by *magnesia*; and appeared so strong to the taste, that I could not have distinguished it from ordinary lime-water: and when I threw some salt ammoniac into the lime which remained, the vapour of the volatile alkali immediately arose from the mixture.

In this experiment, therefore, the air is first driven out of the chalk by an acid, and then, in order to separate this acid from it, we add an alkali which has been previously deprived of its air; by

by which means the chalk itself is also obtained free of air, and in an acrid form, or in the form of slaked lime.

WE have also several processes for obtaining the volatile alkali in a caustic form, which seem to be only so many methods of obtaining it in its pure state, and free of fixed air. The first of these is the separation of the alkali from an acid, merely by heat: an instance of which we have from Mr Margraaf *. He prepared from urine an ammoniacal salt, the acid of which is the basis of the phosphorus, and is of such a peculiar nature, that it endures a red heat without being dissipated. Sixteen ounces of the neutral salt were subjected by him to distillation. The acid remain-

* Mem. de l'Acad. de Berlin, ann. 1746, p. 87.

ed in the retort : and he found, in the receiver, eight ounces of an alkaline spirit, which, he tells us, was extremely volatile, very much resembling the spirit of salt-ammoniac distilled with quicklime ; and no crystals were formed in it, when exposed to the cold air.

A caustic volatile alkali may also be obtained, by mixing salt-ammoniac with half its weight of a caustic fixed alkali, or of *magnesia* which has been previously deprived of its air by fire ; and then submitting these mixtures to distillation : Or, merely by adding any ordinary volatile alkali to a proper quantity of a caustic-ley ; for, in this case, the air passes from the volatile to the fixed alkali, by a superior attraction for the last ; and, by a gen-

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the heat, the compound yields a spirit similar to that prepared from salt ammoniac and quick-lime.

It is therefore probable, that, had we also a method of separating the fixed alkali from an acid, without at the same time saturating it with air, we should then obtain it in a caustic form; but I am not acquainted with an instance of this separation in chymistry. There are two, indeed, which at first sight appear to be of this kind: these are the separation of the fixed alkali from the nitrous acid, by means of inflamed charcoal, in the process for making *nitrum fixatum*; and of the same alkali, from vegetable acids, merely by heat. But, upon examining the product of each process, we find the alkali either
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fully or nearly saturated with air. In the first, either the charcoal or the acid, or both together, are almost wholly converted into air; a part of which is probably joined to the alkali, In the second, the acid is not properly separated, but rather destroyed by the fire; a considerable portion of it is converted into an inflammable substance: and we learn from Dr Hales, that the bodies of this class contain a large quantity of fixed air.

WHEN we consider that the attraction of alkalis for fixed air, is weaker than that of the calcareous earths, and reflect upon the effects of heat in chymistry, we are led to imagine, that alkalis might be entirely deprived of their air, or rendered perfectly caustic,

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by a fire somewhat weaker than that which is sufficient to produce the same change upon lime; but this opinion does not seem agreeable to experience.

THE alkalis do, however, acquire some degree of causticity in a strong fire, as appears from their being more easily united with spirit of wine, after having been kept in fusion for some time. For that fluid, which cannot be tinctured by a mild salt of tartar, will soon take a very deep colour from a few drops of a strong caustic-ley. The circumstances which hinder us from rendering these salts perfectly caustic by heat, are, their propensity to dissipation in the utmost violence of the fire, their extreme acrimony,

mony, and the imperfection of our common vessels. For, before the heat becomes very intense, the alkalis either evaporate, or dissolve a part of the crucibles in which they are contained, and often escape through their pores; which happens, especially, as soon as they have already acquired some degree of additional acrimony, by the loss of part of their air.

THE fusion, also, which they so readily undergo, is well known by chymists, as a strong obstacle to the separation of the volatile from the fixed parts of a compound by fire. Accordingly, in several processes, we are directed to add to the fusible compound, some porous substance, which is incapable of fusion, and will retain the whole
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in a spongy form, thereby to facilitate the dissipation of the volatile parts.

IN order to know whether an alkali would lose a part of its air, and acquire a degree of causticity, when exposed, with this precaution, to the action of a strong fire, I mixed an ounce and a half of salt of tartar with three ounces of black lead, a substance of any the most unchangeable by chymical operations. This mixture I exposed for several hours, in a covered crucible, to a fire somewhat stronger than what is necessary to keep salt of tartar in fusion. When allowed to cool, I found it still in the form of a loose powder; and, taking out one half, I diluted it with water, and, by filtration, obtained a ley, which, when poured into a solution

lution of white marble, in *aqua-fortis*, precipitated the marble under the form of a weak quick-lime: for, the turbid mixture gave a green colour to the juice of violets, and threw up a crust like that of lime-water; and the precipitated powder, collected and mixed with salt-ammoniac, immediately yielded the scent of the volatile alkali.

LEST it should here be suspected, that the alkaline qualities of this mixture, and of the precipitated marble, were not owing to a lime into which the marble was converted, but to the alkali itself which was added, it is proper to observe, that I mixed so small a proportion of the ley with the solution of marble, as made me sure, from certain experiments, that the whole of
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the alkali was spent in performing the precipitation, and was consequently converted into a neutral salt by attracting the acid. The properties, therefore, of the mixture, can only be referred to a lime, as is indeed sufficiently evident from the crust which is peculiar to lime-water.

I was therefore assured by this experiment, that an alkali does really lose a part of its air, and acquire a degree of causticity, by the proper application of heat; but, finding, by several trials, that the degree of causticity which it had thus acquired was but weak, and that the quick-lime produced in this experiment was exhausted and rendered mild by a small quantity of water, I exposed the crucible, together with
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that half of the alkali which remained in it, to a stronger fire, in order to expel a larger quantity of air, and render it more remarkably caustic : but the whole of it was dissipated by the force of the heat ; and the black lead, which still retained the form of a loose and subtile powder, yielded little or nothing to water.

WE learn, then, from the above experiment, the reason why the alkali newly obtained from the ashes of vegetables, is generally of the more acrid kinds of that salt. It never appears until the subject be converted into ashes, and is supposed to be formed by the fire, and to be the result of a particular combination of some of the principles of the vegetable ; one of which

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principles is air, which is contained in large quantity in all vegetable matters whatever. But, as soon as the smallest part of a vegetable is converted into ashes, and an alkali is thus formed, this salt necessarily suffers a calcination, during which it is kept in a spongy form by the ashes, and shews a very considerable degree of acrimony, if immediately applied to the body of an animal: but if the ashes are for any time exposed to the air, or if we separate the alkali from them by the addition of a large quantity of water and subsequent evaporation, the salt imbibes fixed air from the atmosphere, and becomes nearly saturated with it; tho', even in this condition, it is generally more acrid than salt of tartar, when this is prepared with a gentle heat.

BORAX

BORAX has sometimes been referred to the class of alkalis, on account of some resemblance it bears to those salts: But it has been demonstrated by accurate experiments, that we should rather consider it as a neutral salt; that it is composed of an alkali, and of a particular saline substance called the sedative salt, which adheres to the alkali in the same manner as an acid, but can be separated by the addition of any acid whatever, the added acid joining itself to the alkali in the place of the sedative salt. As this conjunction of an acid with the alkali of borax happens without the least effervescence, our principles lay us under a necessity of allowing that alkali to be perfectly free of air, which must proceed from its being incapable of union with fix-

ed air, and with the sedative salt, at the same time : Whence it follows, that, were we to mix the sedative salt with an alkali saturated with air, the air would immediately be expelled ; or, the two salts, in joining, would produce an effervescence. This I found to be really the case, upon making the trial, by mixing a small quantity of the sedative salt, with an equal quantity of each of the three alkalis, rubbing the mixtures well in a mortar, and adding a little water. It is however proper in this place to observe, that if the experiments be made in a different manner, they are attended with a singular circumstance. If a small quantity of the sedative salt be thrown into a large proportion of a dissolved fixed alkali, the sedative salt gradually disappears, and is united to
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the alkali, without any effervescence ; but if the addition be repeated several times, it will at last be accompanied with a brisk effervescence, which will become more and more remarkable, until the alkali be entirely saturated with the sedative salt.

THIS *phænomenon* may be explained by considering the fixed alkalis as not perfectly saturated with air : and the supposition will appear very reasonable, when we recollect that those salts are never produced without a considerable degree of heat, which may easily be imagined to dissipate a small portion of so volatile a body as air. Now, if a small quantity of the sedative salt be thrown into an alkaline liquor, as it is very slowly dissolved by water, its particles

ticles are very gradually mixed with the atoms of the alkali. They are most strongly attracted by such of these atoms as are destitute of air, and therefore join with them without producing an effervescence; or, if they expel a small quantity of air from some of the salt, this air is at the same time absorbed by such of the contiguous particles as are destitute of it; and no effervescence appears, until that part of the alkali, which was in a caustic form, or destitute of air, be nearly saturated with the sedative salt. But if, on the other hand, a large proportion of the sedative salt be perfectly and suddenly mixed with the alkali, the whole, or a large part of the air, is as suddenly expelled.

In the same manner may we also explain

plain a similar *phænomenon*, which often presents itself in saturating an alkali with the different acids : The effervescence is less considerable in the first additions of acid, and becomes more violent as the mixture approaches the point of saturation. This appears most evidently in making the *sal diureticus*, or regenerated tartar : the particles of the vegetable acid here employed, being always diffused through a large quantity of water, are more gradually applied to those of the alkali, and, during the first additions, are chiefly united to those that are freest of air*.

THAT the fixed alkali, in its ordinary state, is seldom entirely saturated with

* Boerh. operat. chym. process 76.

with air, seems to be confirmed by the following experiment—

I exposed a small quantity of a pure vegetable fixed alkali to the air, in a broad and shallow vessel, for the space of two months ; after which I found a number of solid crystals, which resembled a neutral salt so much as to retain their form pretty well in the air, and to produce a considerable degree of cold when dissolved in water. Their taste was much milder than that of ordinary salt of tartar ; and yet they seemed to be composed only of the alkali, and of a larger quantity of air than is usually contained in that salt, and which had been attracted from the atmosphere : for they still joined very readily with any acid, but with a more violent

violent effervescence than ordinary ; and they could not be mixed with the smallest portion of vinegar, or of the sedative salt, without emitting a sensible quantity of air.

As it now appeared that several alkaline substances have an attraction for fixed air, I tried a few experiments, to learn the relative strength of their several attractions.

TWENTY-FOUR grains of *magnesia*, in fine powder, were mixed with five ounces of the caustic-ley, in a small phial, which was immediately corked, and shaken frequently for four hours. The ley was then poured off, and the *magnesia* washed with repeated affusions of water, and dried. It had lost about the
 O half

half of its weight, and, when reduced to a fine powder, was readily dissolved by acids, with an effervescence which was hardly perceivable: the alkali had therefore extracted its air. I also threw some fresh *magnesia* into the ley which had been poured off, and thereby rendered it perfectly mild, and similar to a solution of salt of tartar; so that it effervesced briskly with acids.

WITH an ounce of the mild spirit of salt-ammoniac, I mixed a dram of *magnesia*, in very fine powder, which had been previously deprived of its air by fire; and, observing that the *magnesia* had a tendency to concrete into a solid mass, I shook the phial very frequently. After some days, the powder was increased to more than double its former bulk;

bulk; and when the phial was opened, the alkaline spirit emitted a most intolerably pungent smell. It likewise floated upon water; but was not perfectly caustic, for it still yielded some air when mixed with acids, and also rendered lime-water turbid; neither of which would probably have happened, if I had used a greater quantity of *magnesia*, or had allowed the mixture to remain a longer time in the phial. I now washed out the whole of the mixture into a bowl, and dried the *magnesia* until it lost all smell of the alkali. It weighed a dram and fifty-eight grains, effervesced violently with acids, and therefore contained a large quantity of air, which had been drawn from the alkali by a stronger attraction.

HAVING formerly shewn, that *magnesia* saturated with air separates an acid from a calcareous earth, which it is not able to do after being deprived of its air by fire ; I now suspected that the air was the cause of this separation, because I found that it was joined to the calcareous earth, at the same time that the acid was joined to the earth of *magnesia* ; and imagined, that a pure calcareous earth might possibly have a stronger attraction for acids, than a pure earth of *magnesia*.

I therefore dissolved two drams of *magnesia* in the marine acid, and thus obtained a compound of an acid, and of the pure earth of this substance ; for the air which was at first attached to it, was expelled during the dissolution.

I then added thirty grains of strong quick-lime in exceeding fine powder, shook the mixture well, and filtrated it. The powder remaining in the paper, after being well washed, was found to be a *magnesia*, which, as I expected, was destitute of air, for it was dissolved by the vitriolic acid without effervescence; and the filtrated liquor contained the lime united to the acid, for, upon dropping spirit of vitriol into it, a white powder was immediately formed.

WE must therefore acknowledge a stronger attraction between the calcareous earths and acids, than between these and *magnesia*. But, how does it then happen, that if *magnesia*, saturated with air, be mixed with a compound
of

of acid and calcareous earth, these two last, which attract one another the most strongly, do not remain united; but the acid is joined to the *magnesia*, and the calcareous earth to the air, which it attracts much more weakly than it does the acid. Is it because the sum of the forces which tend to join the *magnesia* to the acid, and the calcareous earth to the air, is greater than the sum of the forces which tend to join the calcareous earth to the acid, and the *magnesia* to the air; and because there is a repulsion between the acid and air, and between the two earths; or they are somehow kept asunder, in such a manner as hinders any three of them from being united together?

THE first part of this supposition is
fa-

favoured by our experiments, which seem to shew a greater difference between the forces wherewith the calcareous earth and *magnesia* attract fixed air, than between those which dispose them to unite with the acid. The repulsions, however, hinted in the second, are perhaps more doubtful, though they are suggested in many other instances of decomposition; but the bounds of my present purpose will not allow me to enter upon this subject, which is one of the most extensive in chymistry.

WE meet also with a difficulty with respect to the volatile alkali, similar to the above. Thus, a calcareous earth, that is pure, or free of air, has a much stronger attraction for acids, than a
pure

pure volatile alkali, as is evident when we mix quick-lime with salt-ammoniac; for the alkali is then immediately detached from the acid. And, agreeably to this, I found, upon the trial, that a pure or caustic volatile alkali does not separate a calcareous earth from an acid. Yet, if we mix a mild volatile alkali, which is a compound of alkali and air, with a compound of acid and calcareous earth, these two last, which attract one another most strongly, do not remain united; but the acid is joined to the alkali, and the earth to the air, as happens in the precipitation of a calcareous earth from an acid, by means of the common or mild volatile alkali.

I remember, likewise, a parallel instance

instance with regard to quick-silver. This metal has an attraction for the vitriolic acid, and, when joined to it, appears under the form of turbith mineral. But this attraction is weaker than that of the fixed alkali for the same acid; for, if we mix a dissolved salt of tartar with turbith mineral, the turbith is converted into a brown powder, and the alkali into vitriolated tartar; which change happens the sooner, if the pure or caustic alkali is used. Yet, if to a compound of quick-silver and the nitrous acid, we add a compound of the fixed alkali and the vitriolic acid, or a vitriolated tartar, and digest the mixture with a strong heat, the vitriolic acid does not remain with the alkali, but is joined to the quick-silver, which it attracts more weakly, composing with

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it a turbith mineral ; while the alkali is joined to the nitrous acid, which it likewise attracts more weakly than it does the vitriolic, and is converted into salt-petre.

FROM some of the above experiments, it appears that a few alterations may be made in the column of acids in Mr Geoffroy's table of elective attractions ; and that a new column may be added to that table, according to the following scheme, where the alkaline substances are all considered as in their pure state, and free of fixed air.

Acids.	Fixed air.
Fixed alkali.	Calcareous earth.
Calcareous earth.	Fixed alkali.
Volatile alkali and <i>magnesia</i> .	<i>Magnesia</i> .
* * * * *	Volatile alkali.
	* * * *

At

AT the foot of the first column, several of the metals might follow, and after these the earth of allum; but as I don't know what number of the metals should precede that earth, I have left it to be determined by further experience.

THE volatile alkali and *magnesia* are placed in the same line of this column; because their force of attraction seems pretty equal. When we commit a mixture of *magnesia* and salt-ammoniac to distillation, the alkali arises, and leaves the acid with the *magnesia*; because this earth, by attracting the acid, represses its volatility; and it seems also to diminish the cohesion of the acid and alkali, and to render them separable by a gentle heat. If the *magnesia* be

P 2

saturated

saturated with air, this likewise, on account of its volatile nature, and attraction for the alkali, is driven up along with it, and makes it appear under a mild form ; and in the same manner do the alkali and air arise from a mixture of salt-ammoniac, and of a crude calcareous earth.

Of

*Of the Cold produced by evaporating
Fluids, and of some other means of
producing Cold ; by Dr WILLIAM
CULLEN.*

A Young Gentleman, one of my pupils, whom I had employed to examine the heat or cold that might be produced by the solution of certain substances in spirit of wine, observed to me, That when a thermometer had been immersed in spirit of wine, tho' the spirit was exactly of the temperature of the surrounding air, or somewhat colder ; yet, upon taking the thermometer out of the spirit, and suspending it in the air, the mercury in the thermometer, which was of Farenheit's

heit's construction, always sunk two or three degrees. This recalled to my mind some experiments and observations of M. de Mairan to the same purpose, which I had read some time before. See *Dissertation sur la glace*, edit. 1749. p. 248, *et seq.*—When I first read the experiments of M. de Mairan, in the place referred to, I suspected that water, and perhaps other fluids, in evaporating, produced, or, as the phrase is, generated some degree of cold. The above experiment of my pupil confirmed my suspicion, and engaged me to verify it by a variety of new trials.

I began by repeating the experiment with spirit of wine; and found, when I had taken the utmost care to have the spirit exactly of the temperature of the

the air, that constantly, however, upon taking the thermometer out of the spirit, the mercury sunk several degrees, and indeed continued to sink so long as the ball of the thermometer continued wet with the spirit of wine. I found also, when the ball began to dry, and the mercury to rise again in the stem of the thermometer, that if the ball was again dipped into the spirit, and immediately taken out, the mercury in the thermometer might be again observed to sink; and that thus, by repeated dippings, the cold produced might be rendered very remarkable. The cold produced was also observed to be still greater, when, between each dipping, the thermometer was moved very nimbly to and fro in the air; or if, while the ball was wet with spirit of wine, it

was

was blown upon by a pair of bellows, or indeed if the air about the ball was otherwise any-how put in motion. If any of these means for putting the air in motion are employed, the repeated moistenings of the ball of the thermometer may be performed by dipping it into the spirit of wine. But, when a certain degree of cold has been produced by a first dipping, that is apt to be diminished by dipping again into the warmer spirit; and therefore the thermometer ought either to be dipped into the spirit, and taken out again very quickly, or, what is still better, the ball of the thermometer ought to be moistened by a feather that has been dipped into the spirit of wine. By taking these methods, I have, by spirit of wine, made the mercury in the thermometer

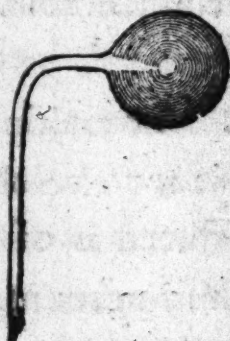
mometer sink from 44 degrees to below the freezing point; and, by employing some other fluids, to be mentioned by and by, I have produced a sinking of the thermometer much more considerable.

IN making experiments of this kind, it is to be observed, that the cold produced is of very short duration. On this account, it is not proper to employ a thermometer inclosed in a glass tube: and it is necessary to employ one having a small ball, which may render it more sensible. But as a small ball occasions the divisions of the scale to be the smaller, a thermometer filled with quicksilver is not so proper for these experiments, as one filled with spirit of wine, having at the same time both a small

Q

ball

ball and a slender stem. What best of all shews the cold produced, and is indeed, with respect to several fluids, quite necessary, is an *air* thermometer. This too will be rendered more convenient, by having the upper part of the tube bent as in the figure annexed; so that the ball may be moistened without the liquor's running down upon the stem and scale.



I have entered into this detail, for the sake of those who may desire to repeat my experiments. Having now said enough on the manner of making them, I go on to observe, that, in this way, I have examined a great variety of fluids; such as,

The

The quick-lime spirit of *sal. ammoniac*,
 The æther of Frobenius,
 The nitrous æther,
 The volatile tincture of sulphur,
 Spirit of wine,
 Spirit of *sal. ammoniac* made with the
 fixed alkali,
 Brandy,
 Wine,
 Vinegar,
 Water,
 Oil of turpentine,
 Oil of mint,
 Oil of pimento.

By each of these employed to moisten the ball of the thermometer, some degree of cold is produced. I dare not however at present determine exactly what is the sinking of the thermometer

Q 2

produced

produced by each. For this purpose, it would be necessary to repeat the trials often, and with precisely the same circumstances at each time ; which I find to be very difficult. In the mean time, I have endeavoured to give a notion of the comparative power of these fluids in producing cold, by the order in which I have set them down, having mentioned that fluid first which seemed to me to occasion the greatest sinking of the thermometer ; and the rest follow in order, as they seemed to occasion less and less.

From the above enumeration I imagine it will appear, that the power of evaporating fluids in producing cold, is nearly according to the degree of volatility in each. If to this we join
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the consideration, that the cold is made greater by whatever hastens the evaporation; and particularly, that the sinking of the thermometer is greater, as the air in which the experiment is made is warmer, if dry at the same time; I think we may now conclude, that *the cold produced is the effect of evaporation.*

I did not think it necessary to diversify my experiments further, by examining a great many fluids, which are manifestly of a like nature, with these above mentioned. I presume pretty confidently, that the several spirituous, watery, and oily fluids, a-kin to these already tried, will be found, all of them, to have similar effects: and, considering how many fluids these classes comprehend; and that, in these already
tried,

tried, the cold produced seems to depend more on the volatility of the aggregate, than on the nature of the mixt ; I was ready to conclude, that all fluids whatever would, in evaporating, produce cold. But I have found a seeming exception. When the ball of the thermometer is moistened with any of the fossil acids, a considerable degree of heat is produced. It is however to be doubted, if this affords an exception. We know that these acids attract water from the air ; and, also, that these acids, mixed with water, always produce heat : It may therefore be supposed, that the heat produced by moistening the ball of the thermometer with these acids, is to be imputed rather to their mixing with the water of the air, than to their evaporation singly.

singly. This perhaps cannot be positively determined, till the evaporation of these acids, in a very perfect vacuum, is examined; which I have not yet had an opportunity of doing. In the mean time, I have made an experiment which I think is to the purpose. To one part of strong acid of vitriol, I added two parts of spring water. When this mixture, which produces a great degree of heat, was returned to the temperature of the air, I used it for moistening the ball of the thermometer, and found it produced a sensible degree of cold, and seemingly a greater than water alone would have done. I need not here observe, that the mixture I used was still a very acid liquor, only so much saturated with water, that it would not now attract any from the air.

Whe-

Whether it would not have had the same effect, tho' less diluted, I have not had time to examine. The experiment, as it stands, tends to prove, that the heat produced by acids, applied to the ball of the thermometer, is owing to the mixture of these with the water of the air; and therefore it is still very probable, that all fluids, which do not immediately affect the mixture of the air, will, in evaporating, produce cold.

WHEN I had proceeded thus far, I began to consider, whether the cold produced in the above experiments might not be the effect of the mixture of the several fluids with the air; and that therefore, to a list of cooling mixtures and solutions, which I was then making

making up, I should now add the several solutions made by the air. By one who supposes the evaporation of fluids to depend upon the action of the air as a menstruum, this would be readily admitted; but, as I knew that fluids evaporate *in vacuo*, as well as in the air, I resolved to suspend my opinion, till I should repeat my experiments in an exhausted receiver.

IN prosecuting these, a number of new, and, to me, curious *phænomena*, have presented themselves; so many, that I find the experiments must be often repeated, and much diversified, before I can give the Society a proper account of them. In the mean time, I shall give you the following facts, already sufficiently verified.

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A thermometer hung in the receiver of an air-pump, sinks always two or three degrees upon the air's being exhausted. After a little time, the thermometer *in vacuo* returns to the temperature of the air in the chamber, and, upon letting air again into the receiver, the thermometer always rises two or three degrees above the temperature of the external air.

WHEN a vessel containing spirit of wine, with a thermometer immersed in it, is set under the receiver of an air-pump; upon exhausting the air, the mercury in the thermometer sinks several degrees. It becomes more especially remarkable, when the air in any plenty issues out of the spirit of wine. As the spirit continues long to give out
air,

air, so it is long before the thermometer immersed in it returns to the temperature of the external air. But, when after some time it appears stationary, if it is then drawn up out of the spirits, and suspended in the vacuum, the mercury sinks very quickly eight or nine degrees ; a good deal farther than it would have done, in the same circumstances, in the air. In the same manner as in the air, the thermometer *in vacuo* may be made to sink lower, by repeated dippings into the spirit of wine: But here these repeated dippings have not so remarkable an effect as in the air; because the dipping cannot be so quickly performed, and the thermometer is therefore more affected by the warmth of the spirit. It is sometimes also affected by a drop of the spirit,

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which

which the ball takes up along with it, and which, as I should have observed above, ought always to be taken away in the experiments made in the air. This experiment with spirit of wine was often enough repeated, to shew clearly, that the evaporation of the spirit *in vacuo* produces a greater degree of cold, than the evaporation of the same in the air.

SATISFIED of this, I have tried also some other fluids, as the quick-lime spirit of sal-ammoniac, and the two kinds of æther. Vessels containing these, with a thermometer immersed in the liquor, but separately, and at different times, were set under the receiver. With regard to the different fluids, the *phænomena* were much the same. Upon
ex-

exhausting the receiver, the fluid gave out a great quantity of elastic air; and, while this happened, the mercury in the immersed thermometer sunk very fast, and to a great length. In our trials, it generally sunk below the scale applied, so that we could not measure exactly how far. In one experiment, before exhausting the receiver, the thermometer had stood at 50 degrees, and, after exhausting, we could judge very certainly that it sunk below 20. In another experiment, made with the nitrous æther, when the heat of the air was about 53 degrees, we set the vessel containing the æther in another a little larger, containing water. Upon exhausting the receiver, and the vessel's remaining for a few minutes *in vacuo*, we found the most part of the water frozen,

frozen, and the vessel containing the æther surrounded with a thick and firm crust of ice.

SUCH a means of producing cold, and to so great a degree, has not, so far as I know, been observed before; and it seems to deserve being further examined by experiments. Till that is done, I do not chuse to give any account of some other remarkable *phænomena* that have occurred in the above experiments, nor to enter into the several speculations that the subject seems to suggest.

SINCE writing the above, I have had occasion to observe, that Mr Richman, of the academy of Petersburg, has taken notice of the effect of evaporat-
ing

ing fluids in producing cold ; but he does not impute it to the evaporation alone. His very exact account of the *phænomena*, and his theory with regard to them, may be seen in *Nov. Comment. Acad. Petropolitanae*, ad ann. 1747 & 1748, page 284.

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